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SOCIAL CHANGE AND MENTAL HEALTH

H. B. M. MURPHY, M.D.

INTRODUCTION

THE belief that social change has an adverse effect on mental health has a respectable ancestry. Esquirol thought that his figures showed that "Les perturbations sociales de cette époque [1830–31] ont exercé leur influence sur la production de la folie, nonseulement par la frayeur et par l'exaltation politique mais par le bouleversement dans la position sociale de beaucoup des individus." [*sic.*](25) Maudsley forty years later, expressed much the same idea when he said, *a propos* the reported rarity of mental disorder in primitive peoples, "[The savage] is extraordinarily conservative . . . he is free therefore from the perils which to unstable natures lie in the excitement produced by revolutionary change and the adjustment to the new relations exacted thereby." (57) The focus today has shifted from political revolution to other forms of change, but the statement in Ruesch's well-known monograph that "The difficulties encountered in the process of acculturation are largely reflected in the statistics of mental disease" (72) is directly in line with these earlier thinkers.

When we come to examine references to this belief, however, we find two different strains of thought which have very different meanings for preventive mental health work. On the one hand we have simple claims or simple demonstrations that change has been a factor in the development or precipitation of mental disorder in specific cases, and no inference is made regarding the effect that the same change may have on other individuals or groups. Such reports invite research into the particular combinations of factors, personal and social, which create a mental health hazard and leave the question open as to whether prevention of the effect might be achieved through some factor other than the change itself. On the other hand there are studies, both empirical and theoretical, which conclude with the inference that social change has the same type

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of effect on most or all people, with the clinical cases being the visible part of the iceberg. Such a viewpoint is implied in remarks about immigrants wearing out earlier than other people, in certain theories of social disorganization, and in statements of the type of Wolff's: "In a rapidly changing society the anxiety-inducing factors are carried along in the traditions of society and outlive the anxiety-resolving factors." (90) According to this view, therefore, it is not particular combinations of factors which create the danger but change itself; and since social change is something which we cannot stop and which, at least in common opinion, is increasing in tempo, it follows that there is virtually nothing that can be done to prevent the effect. Obviously which theory of social change is correct has importance for mental health planning. If social change has a generally adverse effect, then the likelihood of increasing mental disorder in areas of rapid change must be faced, treatment services will have to be stepped up, and research into other factors in mental health must allow for this effect. On the other hand, if social change in itself does not have an adverse effect but is dangerous only when in combination with certain other factors, then it follows that belief in change as the main factor is going to prevent us from attacking other factors where preventive action might have considerable success.

In an attempt to resolve this dilemma the present paper will review what evidence exists regarding the effect of social change on mental health in general, and then examine what concomitant factors may be sharing in the result. If what may be called the *general hazard* theory is correct, then, for every situation in which such groups can be compared, we should expect to see higher rates of mental disorder in people who have undergone a given type of social change than in people who have not undergone it, other factors being allowed for. If, after due allowance for such other factors, there are sufficient instances where social change does not seem to have had any adverse effect, then it seems reasonable to conclude that the general hazard theory has been found wanting and that the other is

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the more probable (though, of course, still other theories are possible, and might prove to be still more applicable).

The main types of change which will be reviewed are migration from one country to another, migration from one region to another within the same country, the changes from war to peace and vice versa, and the adjustment of non-Western peoples to modern Western civilization. Many more examples could be chosen, and it would by no means be agreed by all that the ones so chosen represent true social change. These cases do, however, constitute the main types of general change about which we have epidemiological data, and they are among the ones which have been most commonly cited as examples of social change in the past. Whether particular instances should have been included will be discussed as the need arises; for to stop here and consider the nature of social change and the various definitions which have been suggested for it in the past, would mean that my given task—that of reviewing the epidemiological literature—would never be reached at all. There are serious problems involved in whatever concept of social change one might choose to use, especially when, as in the present case, its relationship to individuals is important; but these cannot be dealt with here. The reader is asked to assume that, unless there are strong reasons to the contrary, all instances of change cited here *are* instances of some form of social change, though not necessarily of social change as limited by any one popular definition.

Since mental hospitalization is that index of mental disorder which is most commonly cited and most easily ascertained, most of this paper will necessarily concern itself with such hospitalization data, and these will be treated first. However, it is recognized that in many ways this index is an unsatisfactory one. In consequence, other indices of mental disorder will also be considered, together, in a subsequent section.

Admissions to Mental Hospitals

Immigration. At one time the striking relationship between

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immigrant status and mental hospitalization seemed, at least to some, clear evidence of the malignant effects of one form of social change. Today the relationship has become quite doubtful, and its meaning equally so.

This change has come about through the analysis of related variables. Before the 1930's, papers were regularly published indicating that the immigrant sections of the United States and Canadian populations had proportionately two to three times more patients in mental hospitals than the native-born sections.[1] Then the work of Ødegaard (63) and Malzberg (57) demonstrated that when variations in age and sex were controlled, most of this difference could be accounted for and that what differences remained applied to virtually all ethnic or cultural groups, to both sexes, and at most ages. In consequence, although the differential was less, its association with the *migration* factor itself seemed strengthened for the racial explanation now seemed impossible, and that factor had previously been the main competitor. However, what was temporarily forgotten was that if differences in age and sex distribution between the native and immigrant sections of the population could account for the bulk of the difference in rates, other differences between these two groups of the population might be able to account for the rest. It had long been known that immigrants differed from natives with respect to average social class, predominant type of residential milieu, years of schooling, and ratio of single to married; and it was also being learned at this time that associations existed between such factors and mental hospitalization rates (77, 26, 27). In consequence, the possibility existed that if these factors were also controlled for, the immigrant/native difference in mental hospitalization rates would disappear completely.

Such a disappearance has not yet been convincingly demonstrated, but two papers have pointed in the expected direction. The first is a relatively recent one by Malzberg, using the 1940 instead of the 1930 Census. (The 1930 Census gave an un-

[1] See reviews in Ødegaard (63) and Gillon (31).

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avoidable bias to his earlier studies in that they had to be related to the Depression, which was liable to hit the immigrant harder than the native.) In this paper (54) he shows that when adjustment for broad rural/urban differences in residence is added to the traditional adjustment for age, standardized mental hospitalization rates for immigrants and natives in New York State become virtually the same (Table 1).

Were we looking only at the mental hospitalization rate and not at the mental disorders for which it is the indicator here, this finding would be convincing, but one cannot help observing (Table 1) that while admission rates generally have become about equal, admission rates for the major psychoses still show a significant differential, with the immigrants showing an excess in both sexes. One very possible interpretation of these findings might be that the relatively recent trend towards seeking hospitalization for the minor disorders (such as the neuroses) had moved much faster among the native-born than among the immigrants. Hence, while this paper is suggestive, it cannot be said to demonstrate convincingly that immigrant mental health is the same as native mental health, once crude urban/rural differences in residence are allowed for.

The second paper is Clark's relatively neglected study of schizophrenia analyzed by nativity and occupational status

Table 1. Ratios of foreign-born to native-born rates of mental hospitalization, in New York State hospitals, first admissions only, 1939-41; standardized for age and urban/rural distribution by sex and main diagnoses.

	Males	Females	Total
1. All First Admissions	95.9	109.1	102.0
2. Dementia Precox	128.8	124.2	127.1
3. Manic Depressive Psychoses	93.7	107.9	103.4
4. General Paresis	108.4	79.4	101.4
5. Alcoholic Psychoses	61.3	90.6	62.3
6. Arteriosclerotic Psychoses	103.5	117.8	110.1
7. Involutional Psychoses	84.9	128.6	113.4
8. Senile Psychoses	97.5	129.7	115.8

Items 1–4 standardized for population of New York State aged 15+ on April 1, 1940; item 5 on population aged 20+ at same date; items 6, 8 on population aged 45+ and item 7 on population aged 35+; all in intervals of five years.
Source: Malzberg (54).

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(16). The broad finding of the paper is that immigrant rates are, on the average, higher than native ones after occupational status is controlled for (at least, this is one of the general conclusions that can be drawn; the paper really focuses on the occupational groups, nativity being an incidental). However, what is interesting at the present juncture is that there were some quite major occupational categories in which the rates for immigrants were either the same or lower than the rates for native-born. Of his 17 categories there were three in which the foreign-born had lower rates than the native-born, three (including the numerically important office worker and domestic servant groups) in which the rates were virtually the same, and four others in which the excess in the immigrant rates was less than what would usually be accepted as significant. Hence although the immigrants had a higher rate on the average, there were quite important sections of the population where this did not appear.

In neither of these studies were education, marital status, or detailed location of residence (e.g. slum or suburb) allowed for, and we have some reason to expect that these would have had an influence on the resultant rates. And another unconsidered factor, which we will be discussing shortly, was internal migration. Consequently, it cannot be said that we know where we stand much better than before. All that we can say is that as more concomitant factors are allowed for, the gap between immigrant and native rates decrease, and that Clark's study—with some groups showing a difference in rates and others showing none—seems better explained in terms of the multiple factor theory than in terms of the general hazard one. However, it must be remembered that his study was only of hospitalized schizophrenia, and hence of limited significance.

These studies have all dealt with the Western hemisphere. When we come to look at studies done elsewhere there is, in general, less detail to be found and the results are equally confusing. In Britain, the incidence of mental hospitalization in immigrant displaced persons has been shown to be extremely

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high (60), age being eliminated as a disturbing variable; and in France, the North African immigrant group probably have a high incidence as well (18).[2] In both these instances, however, there is clearly more than just social change acting as a stress. The D. P. had lost his country and had been through exceptional strain during the war; the African had very low socio-economic status and was faced with a conflict of interests or conflict of loyalities since in his homeland a nationalist struggle against the French was being waged. We cannot, therefore, simply take these groups as people exposed to cultural change and to no more specific stress; the additional stresses which we would like to take into consideration are not ones we can properly allow for.

From Israel (78) and Singapore (61) come contrary pictures, i. e. reports of immigrants having less mental hospitalization than natives. The Israeli data are crude—not standardized for age or anything else—and in the report from which they are drawn one gets the impression that the country's mental hospitalization patterns show some peculiar characteristics. Nevertheless, the figures do indicate that both the European and the African/Asian immigrant groups have lower rates of mental hospitalization than the native-born (78) despite the fact that Israel imposed no real medical limitations on entry, that the social adjustments there would seem to be unusually demanding on immigrants, and that the age structure of the various groups would be in favor of the local-born having the lowest rates. Two counter-balancing points that have to be remembered here are that the immigrant's mental disturbance may be taking a sociopathic instead of a psychopathic direction (crime figures suggest this), and that the local-born are really only a little more established in a cultural tradition than the newcomers.

Nevertheless, the figures as they stand pose yet a further

[2] Eitlinger's admirable study of refugees in Norway, which has just reached me, also shows refugee immigrants to have very high rates of mental hospitalization, but the same remark applies here as to the D.P.'s in Britain. *Psychiatriske Undersøkelser Blant Flyktninge I Norge,* Oslo, 1957.

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challenge to the general hazard theory of social change, and this is also true of the Singapore material. In this last (unpublished) study, population estimates rather than census figures had to be used, which naturally limits the degree to which conclusions can be drawn. Nevertheless, the ratio of immigrants to native-born in mental hospital first admissions was considerably lower than the ratio which has been projected from census and other sources, and when the probable distribution at different age groups within the population was taken into consideration, native rates exceeded immigrant ones at most ages (61).

The lack of unanimity about these findings favors the combination-of-factors theory much more than the general hazard one, but it does not mean that the latter can be ruled out. There are at least two reasons for believing that these findings, even when more thoroughly worked out, would tend to be biased. In the first place it has been argued, with some justification, that mental hospitals tend to be used more freely by those familiar with a country's customs than by immigrant foreigners and that, in consequence, immigrant patients will tend to remain out of hospitals longer than natives. In the second place, immigrants during this century have had to face increasingly strict pre-migration examinations and interviews which have, as a partial intention, the elimination of those most liable to mental breakdown. One may doubt the efficiency of such screenings, but they probably do have an effect, not only during consular interviews but also on the process of self-selection, so that persons having a disturbed family history refrain from seeking emigration lest this history should have to be revealed. Hence it could be argued that immigrants today constitute, not an adversely self-selected group as Ødegaard and others before him have suggested, but a favorably self-selected group, healthier than the average. If this were so—and Dayton's figures showing a relative decline in the crude immigrant/native-born rate differential between 1917 and 1933 could be interpreted as evidence in favor of this argument

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(19)—then the possibility exists that the immigrants' mental health could have been worsened by social change without the rate of hospitalization (or rate of any other sort of disorder) rising above the average for the surrounding population. Since neither of these factors should be operative with internal migrants, i.e. those who move within their native country, one possible way to seek better evidence is to turn to this group.

Internal Migration. Evidence of possible relationship between internal migration and mental disorder does exist, but it is as well to say straightway that it is similarly ambiguous in its conclusions. New and equally grave problems present themselves in the place of those which may have been solved by switching from external to internal migration. The first problem is one for the theoreticians, namely, whether migration within a country—and more especially between such parts of a country as may be expected to have similar attitudes towards mental hospitalization and related matters—constitutes a true form of social change. This question can be left aside for the moment, but obviously any evidence that internal migration is no hazard to mental health becomes irrelevant if internal migration is not accepted as a valid example of social change. The next problem is that of self-selection. While internal migration does not necessitate medical examinations and interviews such as may, by threat or actual operation, affect the nature of an immigrant population, the former is exposed to different biasing factors. In the United States, for instance, there is a tradition that the more able rural youth moves to the town, but against this stand some recent reports of Robins and O'Neal. These show that former patients of an urban child guidance clinic demonstrated, thirty years later, both much more mental disturbance than a control group and significantly more geographic mobility, 29 per cent having left their city as compared with 15 per cent of the controls (71). To gauge the resultant balance of such selective forces on the mental health of both a general migrant population and special sections of

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that population would need an elaborate field enquiry and might not be possible even then. It is thus worth remembering that the following conflicting findings are drawn from different types of social and cultural settings in which different traditions or patterns of migration may prevail, resulting in different selection biases.

The best known study on the subject is that of Malzberg and Lee, and refers to New York State (55). They showed that, of the current population, those who were resident outside the state five years previously (this being the census criterion) had a very much higher rate of mental hospitalization than those who were resident within the state at the same date, this difference applying to all ages, both sexes, urban and rural dwellers, and to both whites and Negroes. Occupation and nativity were not controlled for, and, as the authors themselves noted, the question of undeclared previous admissions to mental hospital in some other state might bias the result in favor of those classed as non-migrant. An undue proportion of the admissions seemed to have been in New York State less than a year, and although this could be intrepreted as reflecting the stress of recent change, other interpretations seem more likely. A study from the Paris region gives similar general results, but without the same allowance for age, type of residence, etc. (82).

Against these (dealing now only with hospitalization studies) must be put the findings of Ødegaard in Norway and an incidental finding in Jaco's social isolation study. Jaco (41), contrasting districts in the same city with high and with low rates of schizophrenia, found that the high-rate area had not more geographic mobility than the low-rate one, but less. His results thus do not refer to total mental hospitalization, and since social class was rather an obvious differentiating variable between his two types of area, the fact that this was not controlled for weakens the significance of the observation for present purposes. It is the Norwegian study which carries the greatest counterweight. There, Ødegaard (64) showed that

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while migrants in Oslo had slightly higher rates than those born and still resident there at time of census or hospitalization, in all other districts of Norway the reverse was the case, the balance for the whole country being definitely in favor of the migrants as the healthier group. Some question must exist regarding these findings since, although 'standardized' rates are used, the type of information on which customary methods of age standardization are based, i.e. the ages of the population-at-risk, does not seem to have been available.[3] But I do not believe an error here could annul the general finding.

As it stands, the evidence is mixed and inconclusive with the weight rather in favor of migrants having raised rates of mental hospitalization, but with no indication whether this would derive from self-selection or from the social change which is presumed to be experienced. One new point that does come out, however, is the difference between Oslo and the rest of Norway as regards migrant/nonmigrant rate differentials. Oslo, Paris and New York are all main cities of their countries. Ødegaard's finding, especially when joined by those from Baltimore and Texas which will be considered below, suggests that the migration to a metropolitan area or main city of a country may have a different significance for mental health from migration elsewhere.

It may be that life in such cities has a different character from that in other cities, or that a different type of person is attracted there, or it may be that patients are drawn there by reports of better psychiatric care. When the Malzberg and Lee book first reached me I was studying diagnostic criteria in the psychiatric department of a New York City municipal hospital and sought to check the last possibility. Of the few patients investigated, three proved to have moved into New York City only a few weeks previously and to have had either premonitions of illness or actual mental hospitalization down South shortly before. In these patients it is my opinion that

[3] There is some discussion of this in Dorothy Thomas' introduction to Malzberg and Lee, MIGRATION AND MENTAL DISEASE (55).

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the move to New York to live with a sister, etc., was largely motivated by the desire to get good care.

War and Peace. The easiest method of avoiding interpretative problems induced by self-selection is to choose changes in which the individual has had little or no say. Two types of social change in which this may be said to apply, and regarding which some data exist, are the onset or cessation of a war, and the invasion of a non-Western people by a Western culture. In both instances there are, as always, further factors which can confuse the issue, but the subjects deserve looking into.

The onset and cessation of a war mark a major social change for a minority of individuals and a more doubtful change for the majority. Those who join the armed forces for the first time then, or who get interned, or who become displaced, or become refugees in a neighboring country, undoubtedly find marked changes in the society with which they are in everyday contact. For the rest of the people, even though there may develop new social attitudes, new institutions (food rationing, for instance), and new patterns of face-to-face relationship (the sharing of bomb shelters), the main social patterns and structure around them remains the same, and hence some might say that essentially no change was occurring. However, psychiatrists at such times have recorded numerous *individual* cases of people breaking down because of the events in the wider society around them even when they were not soldiers, internees, etc., and it is thus appropriate to ask what the statistical effect was on the people at large.

The result is unusually unanimous. In the previous century Esquirol and his followers were noting that the incidence of alienation nearly always dropped with war. In the present century we have data regarding World War II, for France (1A, 1B), Belgium (20), Britain (38, 30), Denmark (79), and Norway (65), all showing a clear decline in mental hospitalizations in the first years, and only Denmark showing any later rise. (In Britain, France and Belgium, of course, developments

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in the later years of the war were obscured by the destruction and diversion of mental hospitals.) The Danish rebound seems quite likely to have been associated with the peculiar problems of loyalty which the Danes faced in these years, and in any case did not seem to raise the incidence rate above the mean long-term trend line (see Svendsen's Fig. I (79)). In Norway, where questions of loyalty were more clear cut, Ødegaard (65) found no later rise and no evidence that the drop had derived from an increase in the interval between onset and hospital admission. In most of the countries cited there were some extraneous factors which might have helped reduce the incidence rate, but all the writers reporting on the subject seem to agree that these were insufficient to account for the size of the drop which was found, and that a genuine drop in psychosis incidence probably did occur. What happens at the change from war to peace, however, is less clear. Denmark and Britain during 1945/46 saw definite rises in mental hospitalization above the average of the wartime years and possibly above what one can guess to be the long-term trend line. In Denmark the rise was of brief duration (79) and clinical data suggest that the fact of change did have some relevance (though here again one must note that for the Danes there was the conflict of loyalties problem which was not an essential part of usual war to peace changes (70)); in Britain the rise was the prelude to the much greater rise of the succeeding years (30), a rise associated with a change in popular attitudes towards psychiatric care and with the introduction of the National Health Service. In Norway, however, Ødegaard's averaged figures show no clear rise above the long-term trend which pre-war years projected (65), and in Singapore there was a long delay before pre-war levels of hospitalization were reached, even though the supply of psychiatric beds was outrunning the demand and though other forms of hospital use (non-psychiatric) very quickly overtook pre-war levels (61).

The upholder of the general hazard theory can make three main points in answer to these findings. He can say that the

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change from peace to war and from war to peace is not a social change in the true meaning of the latter term; it is a change in the society's situation, but not in the society itself. Next, he can say that hospital data, especially at such times, are not valid indicators. Finally, he can say that even if hospital data were valid for this purpose, the correct hospital data have not yet been examined, for the adverse effect may be a delayed one showing itself perhaps only in a much later increase in the degenerative disorders of old age.

To these objections there are no good answers. Indicators of mental disturbance other than hospitalization are virtually not available for this type of change, and studies of the delayed effect of war in general are not only lacking but would be exceedingly difficult to carry out. On analogy with immigration a delayed effect seems plausible, since immigrants do show an excess of mental disorder in old age (Table 1); but, again on analogy with immigration, there is the problem of deciding whether any such hypothesized excess would outweigh the drop in hospitalizations which had occurred earlier. In any case, to know what particular events in a population's earlier history should be incriminated in a rise of psychosis in old age is very difficult. Only on the question of whether the changes from war to peace and from peace to war are social changes or not is there something more to be said. For here one can turn to special sections of the population—military personnel, internees, refugees, etc.—for whom the change was much more striking.

The difficulty with the latter groups is that their experiences during the period of war are what most observers, and most of the subjects, would consider as stressful; and if we find any rise in mental hospitalizations (or in other indices of mental disorder) then it has to be decided whether this rise was mainly associated with the social change, or with the social situation which resulted from that change. Moreover, in the case of the armed forces there is the question whether this was a group successfully selected for superior health—in which case their

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average rates of breakdown should be lower than those for other populations, if other factors were equal—or whether the selection procedures were largely irrelevant.

With the armed forces it seems best to confine attention to the psychoses, since these are the conditions which would almost certainly have required mental hospitalization in peacetime, whereas of the other types of conditions many were acute battle exhaustions; many others were referred to the psychiatric services as a way of getting them out of a unit where they were not accepted; and some of even the seriously neurotic would not have received hospitalization under civilian conditions. In the United States Army there were, according to Appel (3), 45 neuropsychiatric admissions per 1,000 per annum, of which about 7 per cent were psychotic, thus giving a psychosis rate of about 310 per 100,000 per annum. This is at least double the New York State civilian rate for males 20–39. Moreover, a relatively high proportion of the psychotic breakdowns appeared to have occurred in the early period after induction. Seventy per cent of a sample of psychotic soldiers discharged before proceeding overseas and studied at Bellevue, had broken down in their first 5 months (40), and 50 per cent of Paster's home-base cases broke down in their first year (67). No incidence rate can be calculated for these early breakdowns, but it seems almost certain that it was much higher than would have occurred in the same population if it had not been inducted, and that it was the social change rather than the conditions met with which was the major environmental factor. Seventy-seven per cent of a sample of early psychotic breakdowns studied by Will (87) had never previously been away from home, as compared with 35 per cent of controls, and Klow has pointed out (48) that the type of disturbance found in such patients, though usually called schizophrenia, was more an acute, confusional, often paranoid state showing rapid and complete recovery. (Other reports dispute this last finding, but the difference may relate to the speed with which treatment was instituted.) However, these United States results

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are not necessarily typical. In the Indian Army in peacetime, the incidence of psychiatric admissions of all types was only 1.06 per 1,000 per annum for Indian troops and 2.80 for British troops. The highest psychiatric admission rates ever reached during World War II (on the Arakan front, where conditions were unusually anxiety-provoking) by these two groups was 6.6 and 12.9 per 1,000 (6, 88).[4] These are very different figures from the 45 per 1,000 which Appel reports for the United States Army (3). While, therefore, induction into the army would, from United States data, appear to be associated with a raised incidence of hospitalized psychosis despite efforts to screen out the pre-psychotic, data on troops from other cultures do not necessarily confirm this finding. In regard to the Indian peacetime and early wartime figures, a possible source of error may lie in the fact that commanding officers could discharge sepoys from the force without stated reason within their first six months of training, and hence might have diverted certain cases from medical hands. Nevertheless it is possibly significant that no papers on acute psychotic breakdowns in soldiers during their early training period seem to have been published by British or other European workers.

Regarding the change from war to peace as it affects soldiers and ex-soldiers, there is virtually no evidence touching on mental hospitalization. Clinical papers exist and there is Curle's well-known study of readjustment in a broader sense (17), but no statistics exist on the incidence of major breakdowns in the demobilization period. On delayed effect (or lack of effect) something does exist, however. In 1953, Canada's Dominion Bureau of Statistics included in its tables of statistics on mental hospitalization, the numbers reported to be veterans, this having been a specific enquiry on their reporting card. Table 2 below shows the more interesting items. They can be compared with the statement by the Encyclopedia Canadiana that "forty per cent of the male population aged 18–45 spent some

[4] Estimated comparative *psychosis* incidence in the two groups during the Arakan campaign stand at 177 per 100,000 Indian troups p.a. and 63 per 100,000 British (61).

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time in the armed forces before the war ended" (i.e. World War II, not the Korean War). On the face of it this table suggests that while veterans probably produced more than their share of mental hospitalizations for certain types of neuroses, they produced, in 1952, less than their share of the functional psychoses. In 1952 the mean age of Canadian veterans was probably about 32, which is also a common mean age for onset of schizophrenia. If, therefore, the reporting hospitals completed this item conscientiously, then it would appear that although veterans were producing more than their share of hospitalized minor mental disorders (first admissions) they were producing much less than their share of the more serious disorders, and of mental hospitalizations generally.

Table 2. Percentage of veterans reported by certain diagnostic categories in first admissions to Canadian mental hospitals, 1952.

Diagnostic Category	Per Cent of First Admissions, Males Only
Hysterical Reaction	37.5
Somatization Reaction	36.8
Anxiety Reaction	31.0
Total Psychoneuroses	*23.5*
Alcoholism	28.1
Psychopathic Personality	23.9
Total Personality Disorders	*25.2*
Alcoholic Psychoses	16.9
Paranoia	16.1
Manic-depressive Psychoses	8.4
Schizophrenia	13.8
Total Psychoses	*11.1*
Total First Admissions	*11.8*

Source: Abstracted from Table 34 of the [Canadian] Dominion Bureau of Statistics, *Mental Institutions. Report for 1952*

The final type of social change to be considered here in connection with war is commitment to, and later release from, a P.O.W. or civilian internment camp. From the worst camps any information we may have about hospitalization and psychoses is probably of doubtful value, both because services were likely to be inadequate and because certain types of the mentally disturbed might be unlikely to survive long enough to be diagnosed and recorded. Yet from other camps we have in-

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formation which is fairly reliable and which suggests that major mental disorders, at least during camp life, were less than normal. Thus, from the P.O.W. camps in the Singapore area during World War II the incidence rate for psychoses works out at approximately 40 per 100,000 per annum, plus a further 13 if one includes the reactive depressions (8). In the civilian internment camp in the same area the rate works out at 57 (69). For the P.O.W. camps for Germans in the United States, Gottschick (33) reports all forms of mental breakdown to be about the same as for the pre-war civilian German population, but schizophrenia to be less. In the displaced persons camps under this writer's administration from 1946 to 1949, the incidence of mental hospitalization works out at about 20 per 100,000 per annum (though minor mental disturbances were common) and correspondence with the Bundesgesundheitsamt suggests that for at least one other province (Land) similarly low rates were obtained. In the worst concentration camps the picture was different, but what has struck many trained survivors was the disappearance of certain forms of neurosis, the rarity of suicide (at least, in open form) and sometimes the rarity of the classic chronic psychoses. In Theresienstadt, admissions to the camp hospital with a psychiatric label were common, but these were sometimes subterfuges for getting someone extra care (49). On the whole it seems true that the incidence of major mental breakdowns in such situations was below average.

After release from such camps the picture may change, though data are regrettably scarce. The displaced persons who showed so little hospitalization while in their camps had a quite excessive rate on resettlement in Britain (60); one group of 2,600 Danish KZ survivors produced five suicides (if these be counted as psychosis equivalents) in four years (37); and a reactive depression was reported to be relatively common in those released from the Singapore civilian internment camp (69): that is the positive evidence. On the other hand, United States studies of repatriated P.O.W.'s, while reporting anxiety

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and adjustment reactions, do not report any excess of major breakdown (10) and it is surprising that European countries (other than Denmark) have not reported any special problem in their ex-concentration camp people. Israel would seem to be one of the best sources from which to obtain a definitive answer, but satisfactory data there do not exist since their mental hospitals were for many years inadequate to their needs. In 1949, when the writer visited that country, psychotic reactions in resettling middle-European Jews (most of them from either ex-KZ or ex-D.P. camps) were reported to be high. In 1956, however, Sunier, as previously noted (78), found that although the immigrant from Europe had a much higher rate of hospitalization (patients *in* hospital; not admissions) than the Jewish immigrant from Asia or Africa, the local-born Jew had a higher rate still, both for mental hospitalization in general and for schizophrenia.

That is all that seems relevant to report on the relationship between mental hospitalization and the social changes accompanying the onset and cessation of a war. Once again there are conflicting findings, but on the whole the conclusion must be that the rate of mental hospitalization is not as a rule raised for groups who have experienced this type of change; rather it tends to be lowered. Before discussing the significance of this, however, it will be well to deal with the final instance of social change to be discussed here, and so have all the evidence relating such changes to mental hospitalization on the table.

Acculturation to Western Civilization. There are considerable difficulties in weighing mental hospitalization data referring to the Westernizing of non-Western peoples. It is rare that the availability and use of mental hospitals is the same in a (usually rural) primitive group and in a (usually urban) Westernized group from the same background; questions of what to regard as a major mental disorder are quite acute; and such matters as social class and education, which are differentially distributed between the two groups (if the con-

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cepts, as commonly used, have relevance for such a comparison), are virtually never able to be allowed for. Nevertheless there are, from scattered sources, a number of instances where a Westernized or semi-Westernized group has been shown to possess unusually high rates of mental hospitalization or its equivalent, while a less Westernized section of the same people shows no apparent excess. If one accepts Moloney's report on the people of Okinawa (59) as indicating, at the very least, a less than average rate of psychosis, then the very high mental hospitalization rates which Okinawan immigrants to Hawaii show are striking (47). Again, the psychosis rate which Gans (29) uncovered among Javanese physicians who had all gone through Western schools and colleges, is strikingly higher than the rate among Javanese in the Netherlands East Indies army, being high for any population anywhere. In Singapore the English-educated Chinese male aged 20–49 had higher rates of mental hospitalization for nearly all categories of disorder than the Chinese-educated male (though not more than the illiterate) although the former was of higher average social class (61). Seligman has described classical types of psychosis occurring in association with Christian missions among the Papuans and Melanesians and has denied even hearing of such conditions in his many years of work among the same peoples in their original villages (75).

To weigh against these, I can find only doubtful evidence that the Westernized section of any non-Western people has proportionately less mental hospitalization, or its equivalent, than the less Westernized section, or even a rate that is no higher. Among Indian soldiers, the English-educated seem to have shown more neurosis but definitely less psychosis than the ordinary sepoy (88); but the question of rank and other military training also comes in here, as well as the impact of army life. In Tanganyika, according to Smartt (76), the proportion of Christian and Western-educated among psychiatric hospital admissions is not higher than among nonpsychiatric hospital admissions, but how accurately he was able to work

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this out one does not know. Finally, in Hawaii the part-Hawaiian have significantly lower rates of mental hospitalization than the full Hawaiian (73), and while the latter cannot by any means be considered to be untouched by Westernization it could be argued that they are less Westernized than the part-Hawaiian. However, I doubt whether the distinction between part-Hawaiian and Hawaiian would be as carefully recorded by the mental hospital staff as it is at the census, and any tendency to record a patient as Hawaiian because he looks or speaks the language would markedly affect the apparent incidence rate for the full Hawaiian group, who are a small minority today.

It would thus seem that we now had at least some reasonably unanimous evidence incriminating social change, or at least incriminating one form of social change. For the balance of the evidence suggests that when non-Western peoples come in contact with Western civilization, and must to some extent adjust to it, mental hospitalization rates are higher than when the contact is slight and calls for little or no basic adjustment. However, there is another way of looking at such data, namely by comparing the rates of the transitional group, not with some non-Westernized people, but with a fully Westernized group, i.e. with Americans or Europeans themselves. Such a comparison seems equally legitimate and would seem to lead to a very different conclusion, namely that mental disorder, as measured by hospitalization, rises as one becomes more 'civilized'. This is an old idea, of course, and much argued against, but it is largely true that the same data that can be used to argue for the harmful effects of cultural change can equally be used to argue for the harmful effects of Western civilization. The part-Hawaiians may have a hospitalization rate which is lower than the full Hawaiians, but it is lower than that for the Caucasians also (73, 85). The detribalized African in Kenya may have a rate (13.3 per 100,000) which is higher than for the tribalized, but, as Carothers (14) remarks, this rate is still very low by Western standards or by the standards of the White in Africa,

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although, "as it is unlikely that employed natives would not be certified if insane, this figure is probably a fairly true measure of the insanity in these people." In Singapore the rate for the English-educated section of the Chinese population is probably lower than for the Europeans there, and the rate for Eurasians (who are usually considered to be especially in a position of social instability and change) is definitely lower than that for the Europeans (61). In Cape Province, mental hospitalization for the Cape Coloured is lower than that for local whites, even when only urban sections of the population are compared (50). In the southern United States the rates for Hispano-Americans are lower than for Anglo-Americans, even though the latter carry the dominant culture to which the former are slowly having to shift (42). These are facts which make the sort of evidence cited in the previous paragraph much less clearly in favor of the general social change hypothesis. It is possible, of course, to argue that in the cases cited the transition group had reached a certain stability of cultural admixture, or that the Western group against which it was being compared was an unrepresentative one, selected or exposed to special stresses. It is also true that there is much more information which one would like to have in each instance, so that groups could be more closely compared. Nevertheless, it must be recognized that the evidence regarding cultural transitional groups, although much more unanimous than any other which has been considered here, is open to more than one interpretation and does not simply support the belief in a relationship between mental disorder and social change.

Mental Health Survey Data

Relatively few mental health surveys, using this term in its broadest sense, give data of relevance to the question of social change, and since such surveys are not numerous in any case there is little material to present here. If one were to use more impressionistic studies of self-selected populations, then there would be much more to report, but such studies concentrate

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on the disturbed sections of their populations and usually leave out of consideration the possibility that just as much mental disturbance might be found in a population which has not had the experience under consideration. (This does not mean that the latter type of study is useless, but it means that its use is confined to determining what sorts of breakdown occur, not what their relative frequency is. And it is this last point which is being studied at the moment.)

Regarding immigration, the only mental health survey currently available is that of Weinberg in Israel, although the Midtown study should offer something in the near future. Weinberg, a psychiatrist, interviewed 280 immigrants from Holland to Palestine, found their health and adjustment generally good, and noted no special features about their adjustment period other than increased need for sleep; but he did also record that 44 per cent of them felt more nervous than in Holland, whereas only 12 per cent felt less nervous, the nervousness being inversely related to degree of success (86). The question, of course, is whether this self-assessment of nervousness has any relevance for mental health.

In respect to internal migration, information is somewhat fuller, since four main surveys mention it. Bremer's north Norwegian community study shows that 'immigrants' had a significantly higher rate of mental abnormality than 'natives', the difference lying wholly in the under-40s (9). This apparently contradicts (or corrects) Ødegaard's hospitalization finding on internal migration in Norway. But it must be noted that Bremer states his 'immigrant' group to be an abnormally selected one, thanks to local wartime conditions, with many of the disturbed coming from Finmark—a region of Norway with the highest mental hospitalization rates (64). Whether Bremer's findings are valid for present purposes is therefore doubtful, and the survey in nearby Finland reverses the position again. There, in the 1930s, it was found that the province with the highest inward migration (Viipuri) was the one with the lowest rate of mental disorder, this last term excluding

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mental deficiency and epilepsy (45). No age adjustments were made in that study, which was based on hospital plus key informant sources, but if the migratory trend was, as is the more usual, one of adults rather than of families with children, then correction for age should increase the difference in rates rather than reduce it. Of course what is offered here is only indirect evidence, and it could easily be that the province which attracted most migrants was the one which possessed to the highest degree those features which benefit mental health. By itself, therefore, one cannot put much weight on this survey. The third study, that on the Eastern Health District of Baltimore, produced the oft-quoted conclusion that "There is a definite inverse relationship between the prevalence of mental health problems and duration of residence in the house, but no such relationship can be demonstrated with length of residence in the city." (80) This seems much more direct evidence on the matter, but Dorothy Thomas has pointed out (55) that the method of conducting the survey was such, probably, as to exclude from final consideration the most mobile section of the population. Finally, Gartly Jaco's Texas study (43) offers similar negative findings, but it is open to criticism from another source. He found that there was no significant difference in psychoses rates between those born in the state and those born elsewhere, whether Hispano-American, Anglo-American or 'non-White', but his data only cover psychoses here, and his method of survey excludes those who had not sought treatment. There could thus be a marked difference in neuroses rates, or in the rates of untreated psychosis, which this particular method of approach would not show.[5]

[5] A fifth study of great relevance was overlooked in the first draft of this paper. Martin, Brotherston and Chave investigated the incidence of mental disturbance in a new housing estate [= development] outside London, using hospital admissions, psychiatric outpatient clinic visits, general practitioner consultations, and sample interviewing of some 750 families. A nonmigrant control group was not studied, but from comparison with previous studies the authors conclude that virtually every indicator of mental disorder used showed the new estate dwellers to be less mentally healthy than expected. They are undecided whether to attribute this finding mainly to the social change or to the relative isolation of life in such a development, but they note that symptoms were more frequent in the most recently migrated than in the longer

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With none of these surveys being satisfactory for present purposes it is to be hoped that the Stirling County and Midtown studies—neither of which have been published in adequate detail at time of writing— will help clarify the situation. All that one can say is that present data, none of them satisfactory alone, nevertheless do tend to suggest that mental disorder is not increased in migratory populations.

If there is no mental health survey on the transition from peace to war known to the writer which seems useful to cite here, there are two on cultural change in non-Western peoples. The better known is that of Tooth on the Gold Coast (81), who sampled a number of districts for cases of major mental disorder revealed through census or by local chiefs, but who is quite doubtful about the representativeness of the result. He found a significantly higher rate in one district with four large towns in it than in the other districts, and gives many clinical descriptions of cases in which Western influences may have had a precipitating action, but he regards the difference in incidence as reflecting nothing more than differences in ease of ascertainment. He concludes that ". . . this survey provides no evidence in support of the hypothesis that psychosis is commoner in the Westernized group than in the rest of the population . . .", although ". . . it may be that among the neurotics and minor forms of personality disorder the exposure to Western culture has a more unsettling influence." Carothers, commenting on this finding, suggests that it might be due to a greater mortality among the non-Westernized insane group (so that a difference in incidence might exist even though no difference in prevalence) but continues to insist that what is remarkable is not the relative levels in Westernized and non-Westernized Africans but the low overall level of mental disorder in Africans whether they are Westernized or not (14).

resettled. Without presenting data they suggest that "psychological maladjustment was exceptionally common among children on the estate immediately after rehousing, but that a large measure of stabilization occurred later." A local, subcultural, factor in this case may be the strong clannishness of London East End families, which the rehousing disturbed. [56]

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The final survey to be cited here is one in which the level of prevalence was by no means low, and where a form of change does possibly seem implicated, though whether one should take it as representative of social change in general is very questionable. Van Loon followed up a psychiatric census of North Sumatra in 1918 by visiting and examining patients in different sample areas, and came out with very interesting data (84). He found that major mental disorders were extraordinarily frequent in certain areas and quite infrequent in others although the high prevalence areas were precisely those in which one would have expected the poorest ascertainment, since they were at the heart of a culture which had recently been defeated by the Dutch after a long war, whereas the low prevalence areas were those in which the Dutch administration (and plantations) had been most quietly welcomed. Further, a re-analysis[6] of his data (44) shows that in the high rate areas, male cases greatly outnumbered females (110 to 40) and were predominantly schizophrenic although an impressionistic medical report on the same people made when the war was in its early stages states that mental disorder was infrequent, occurred mainly among females, and was mainly of an acute confusional type. If the earlier report is accepted, then it seems highly probable that there had been an increase in male schizophrenia and male mental disorder generally, between the time when these people were apparently successfully resisting the Dutch and the time when Van Loon saw them as a defeated tribe whose males had lost their *raison d'etre*. (Apparently the Dutch did not recruit them into their own local army, as the British had successfully done after the Sikh rebellion, and Van Loon describes the males as listless and inactive, while the females had taken over the running of affairs.) This seems a possible example of mental disorder increasing in response to social change, but one of a special character. Not only was this people suffering military defeat (not in itself necessarily a precipitant of mental disorder, as we know from elsewhere)

[6] Given in full in reference (61).

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but for the lifetime of the current generation the persistance of the war had meant that the formerly successful warrior ethos could neither be quietly abandoned nor be actively tested against various more adaptive alternatives. These males could, therefore, be considered to be trapped by the impossibility either of succeeding in terms of their traditional culture or of finding an alternative field to succeed in.

Psychological Test Evidence

The picture which has developed so far is, despite all its complexity, fairly clear in one major respect. There are almost as many studies which suggest that social change leads to *no* increase in mental disorder, or even to a decrease, as there are studies suggesting than an increase is directly traceable to such cause. This would be satisfactory backing for the belief that social change has an adverse effect when in combination with certain other factors but no adverse effect in general, were it not that the minor mental disorders have scarcely been dealt with at all. Not only the hospitalization studies, but the mental health surveys which have been cited have dealt quite disproportionately with psychoses only, or frank 'insanity.' Yet the few times the lesser disorders have been mentioned there has usually been some impression that they were frequent in the groups experiencing change even when the psychoses were not. They were the main part of Bremer's Norwegian migrant disorders; they might be inferred to be slightly raised in Weinberg's Jews; they were exceedingly frequent in some refugee camps even when the mental hospitalization rate was low; they may, according to Tooth, be raised in the Westernized African. Moreover, such disturbances have been reported as excessively frequent in Asian and African students at Western universities;[7] in the Christian Bataks as opposed to the pagan Bataks;[8] in ex-P.O.W.s (17); and in certain groups of immigrants (60). Evidence of the latter kind is

[7] Personal communications from various student health services.

[8] Personal communication from Professor P. M. van Wulfften Palthe.

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not all in the one direction. There are, for instance: Aubrey Lewis' finding that neuroses did not increase in Britain during the first years of World War II except in those who had been bombed (51); reports on the disappearance of neuroses in persons confined in concentration camps (often with their reappearance after release (8, 74)) and Russell Frazer's wartime finding that "Workers who had during the war changed their residence or their work, often under compulsion, had no more illness than the rest" (28). However, the latter evidence tends to be associated especially with war, and it might be argued that the changes which war brings are not permanent and hence do not have the same effect as other changes. Some further exploration is therefore desirable with regard to lesser degrees of mental disturbance. If, in general, certain symptoms were regularly found associated with social change then the question would still have to be raised as to whether these represented a pathological state or a transient and essentially healthy adjustment phase (for instance, it might be asked whether serious personal loss can be healthily adjusted to without some depression), but that need not be dealt with before its time.

Mental health surveys do not provide more than what has already been mentioned, but a source of checking does exist in population samples that have been given psychological tests of the projective or symptom-list type. The validity of such tests as measures of mental health is still debated, but when the same test is given by the same administrator to two or more groups having different exposure to some variety of social change, then the results are worth examining. Since it is a marginal area of the present subject, however, the literature has not been searched for all available evidence and only sufficient examples will be cited to indicate on what side of the question such evidence is likely to fall.

Studies do exist where a group having seen much social change is shown to be less healthy than some comparable group which had experienced les change. Hallowell's Rorschach stud-

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ies of different groups of Ojibwa, for instance, showed that those which had made the most rapid strides towards apparent acculturation to modern United States society showed signs of regression or "frustration of maturity" which less acculturated groups did not (36). However, it is relevant to note that the apparent change made by the more rapidly moving had no depth, and that true acculturation to modern society was being frustrated by white attitudes. Another example is Grygier's study, using a battery of tests, of Polish and Jewish displaced persons and KZ survivors immediately after the war (35). His findings suggest marked psychopathic traits in this group; but, of course, traits which suggest psychopathy in peacetime were those which it had been necessary to acquire for survival in Nazi-occupied Poland and it took some time to discard such traits after the war ended.

Opposed to these studies and more relevant for present purposes are some which show no apparent difference in mental health as between two relevant groups. One such is the Algerian work of Miner and DeVos. They compared residents of a small and relatively isolated oasis in the Sahara with people living in Algiers who had been born and brought up in the same oasis (21). The mean Rorschach scores for the two groups are significantly different in a number of items, but these differences relate to the modes of expression of maladjustment or adjustment only. The 'maladjustment index' was much higher for the city Arab than for an American 'normal' sample, being as high as an American neurotic sample gave, but the oasis Arab group was not any lower. The city Arab showed indications of aggressive feeling which were presumed to be related to his situation *vis-á-vis* the French, but the oasis Arab showed disturbances in a different direction, some of which the city sample appeared to have less of. A second study is a Rorschach one of Spanish and English children, the Spaniards being refugees from the war in Spain and the English, evacuees (83). Differences were again found between the groups, but again these were traced to their different cultural backgrounds;

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the Spaniards' presumably greater experience of change is not revealed in any signs of greater suspect pathology. A third example is the writer's own work with Malayan students, using Sentence Completion Tests, Rorschach, and some other so-called 'tests of neuroticism'.[9] At the present stage of analysis no signs of greater mental disturbance can be found in the tests of students coming to the British-style university from up-country towns and villages in Malaya than in those coming from the three main centers of population and Western influence. This result may be affected by the fact that it probably needs more intelligence and drive for a village boy to reach university than for a city one, but at time of writing this factor does not seem to have been relevant.

These illustrations suggest that in as far as psychological tests are able to reveal lesser degrees of mental disturbance, such lesser degrees seem to be no more consistently raised in the presence of social change than are the major disorders. The predominant impression is that they may be raised in association with change in certain contexts, but not significantly raised in others.

Summary Regarding the Competing Hypotheses

A backward glance over all that has so far been reviewed is now in order. What we have found is that in one type of social change situation, and one only, there is fairly unanimous agreement that those undergoing rapid change show more mental pathology than others from the same background undergoing less change. This situation is the Westernizing of non-Western peoples. However, serious doubts can be expressed whether the added pathology should be attributed to cultural change as such, or to the nature of the state towards which the change is directed, since those people who are the embodiment of that state have, apparently, a level of mental ill health which is higher still. In all the other types of situation which we have considered the evidence is more than ambivalent; it suggests

[9] Unpublished.

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strongly that mental health, as measured by the indicators forced on us, may be worsened in some situations of change but bettered in other situations. The difference between these two classes of situation does not seem to relate to the degree of change being experienced. Possibly it may in part be related to the speed of such change, but mainly the difference seems to relate to factors which are virtually independent of the fact of change itself. Accordingly, I think we can say that the sum of the evidence is strongly in favor of the associated factors theory, and strongly against the theory of social change as a general mental health hazard.

Given that conclusion, the question now arises whether any general rules can be formulated regarding types of situation where change becomes traumatic.

Associated Situational Factors

Personal Factors. An unusually consistent finding in the literature on mental disorder and social change is the fact that there is more likely to be an excess of mental breakdown in youth and in old age than in the years of adult maturity. In Ødegaard and Malzberg's early studies of immigrants it was found that the greatest excess in hospitalization was among those aged 20–29, and in those over 70. (57, 63) In Singapore a proportionate excess of immigrant over native-born admissions was probable in the age group 15–25, and again in the age group 55 +, while in the intervening age group immigrants probably produced less mental hospitalization than the native-born (61). In wartime Norway it was again the old and the young who showed a relative rise in admissions, as compared to peacetime, whereas admissions in the middle adult years showed a relative decline (65). Apparent exceptions do exist to this general pattern—Bremer's 'immigrants' (9) and the Okinawans in Hawaii (47)—but these may be due to particular local conditions. Youth and old age do seem to be factors whose association with social change is more than usually likely to be traumatic.

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On the other hand, childhood appears to be unusually immune to pathological sequelae to broad social change. The children of immigrants are reported to show less delinquency (at least, in the U.S.) than the children of native-born parents (72). Wartime displacement in Britain was, as a general phenomenon, reported to be surprisingly little associated with childhood disturbances (83). Regarding the refugee status, Poslavsky and Wiegersma found themselves unable to report any detrimental effect being manifested in D.P. camp children even though they had set out specifically to find this, and even though the amount of mental disturbance in the adult inmates of these camps (who were by this time, 1954, a highly selected remainder) was high (68). It is a broad finding from many forms of change that children remain undisturbed provided only that they remain within their family and that the family, in its functioning, does not change. What children are quite easily disturbed by, however, is major change within the family (or family substitute) itself. Disturbances in children in wartime Britain were nearly always associated with the breaking up of the family, as when the children were evacuated while the mother remained behind (24). Hospitalization of a child, as is well known today, is similarly hazardous (7, 22). Moving to a suburb from a metropolis is, according to R. E. and K. K. Gordon, mainly disturbing to male children, not female, for the apparent reason that the father must spend much more of his time away from home and hence deprives his sons of a necessary model (32). Refugee children who had lost or who had been separated from their parents were, on average, much more disturbed than those who managed to stay with them, and of those who were separated from their parents but who managed to form gangs, separation from the gang proved a disturbing action which the promise or actuality of renewed home life could not negate. (60)

This contrast between childhood's relative immunity to harm from certain types of social change and relative vulnerability to other types is not resolvable by saying that the

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parent-child relationship is a unique one that cannot be put in the same class as other forms of social relationship or situation. The youth of 20 still usually has his parents, but he has become much more vulnerable than the child of 10, and the child of 10 who has lost his parents but found a peer group milieu is quite vulnerable to a disturbance of that milieu. One could say, of course, that this proves any attempt to group different forms of social change together to be pointless, but that would be, in my opinion, to go too far. An interesting, general, explanation can be hypothesized through the assumption that social change only has the possibility of creating mental disturbance when it occurs within what the individual perceives to be his 'own' or 'true' society. As Lois Murphy has said in another context, ". . . when an individual feels himself part of society, he may not be exposed to shock as long as the part of society which he is incorporated in, and is part of, is undisturbed." (62) To a baby, the 'true' society which it can perceive itself to be part of may consist only of the mother and itself; to a child it may consist only of the family; to an adult it may consist of face-to-face contacts only, or his whole nation, or may even be largely imaginary. Which is the perceived 'true' society will depend partly on the personality, partly on what society teaches. It seems probable, however, that with education and experience what one perceives as one's 'true' society enlarges, and hence makes one more sensitive to changes in the wider society around one (sensitive not necessarily with the meaning of vulnerable) while at the same time making one less intensely sensitive to changes in one's most immediate circle.

This factor of personal perception of society is one which, if valid, might explain not only the observations on childhood just considered, but a number of other findings. For instance, it might be the explanation why there has been reported relatively more disturbance in P.O.W.'s after their release than when they are still in captivity. One could hypothesize that the internee or prisoner-of-war, on first entering his camp, does

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not regard his 'true' society as disturbed, since that 'true' society still exists in his home and in his mind; and the change, though objectively great, has thus an external character for him. (The same would be true for many recruits entering a military unit.) However, by the time these individuals are released, it seems likely that their perception of society will have shifted so that the camp community or unit itself becomes their 'true' society, and when disbandment of the latter occurs they can no longer regard this 'true' society as continuing to exist undisturbed elsewhere. The dispersal of the community is a serious disturbance of their 'true' society, as they now perceive it, and one which is too obvious to be denied. Hence restitution to their former society, which on common sense terms should bring ease and joy, would on this hypothesis be a social change whose impact would be fully felt. This, I believe, is part of what Curle has described, and the fact that the "Civil Resettlement Units" were so successful can in part, though only in part, be ascribed to the fact that they helped merge the one society into the other (17).

Other findings where this factor of personal perception might be at work are the Baltimore, Texas, and Singapore reports on migrants. It can be hypothesized that these migrants did not regard their neighbors as important members of their 'true' societies, whether they had intercourse with them or not, and hence that moving out of reach of these neighbors was not a social change in the perceived sense. The migrant to Texas may—and this is only hypothesis—be regarding his 'true' society as comprising the whole American nation and hence be finding that one neighbor can substitute for another provided only that he is American. (In overseas communities it has been reported that Americans are particularly anxious to be beside other Americans, showing more clannishness than many other expatriate peoples and getting disturbed when they must live among non-American neighbors.) (2) The Singapore Chinese, almost certainly, regards his neighbor as of little importance compared to members of his great family or clan, plus its con-

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nections, and he is thus likely to perceive his 'true' society as being unchanged provided that family is intact and there are members or connections from it within reach.

More concrete support for a belief in personal perception of society as a factor is to be found in the Cornell medical histories study, to be discussed below (39). However, there are also instances where the factor does not seem to be relevant or where its existence would even seem to be disproved, unless some factor could be evoked. The Okinawan, who has such high rates of mental hospitalization in Hawaii although low ones in his homeland, should be as able to retain a mental image of his 'true' homeland society when he migrates as the Japanese can. The East Indian who migrates to Singapore and Fiji and shows comparatively high rates of mental hospitalization in these two places (61, 5) should be as able to manipulate his perception of his 'true' society as the East Indian who goes to South Africa and to British Guiana and there produces, relative to the local and white populations, quite low rates of breakbown. (50, 23) Hence while perception of society is one personal factor which probably works together with social change to produce or to prevent mental breakdowns, it is by no means the only concomitant we must look for.

The deterioration of faculties in old age is another personal factor of some relevance. It is probably the major one affecting the earlier finding that when change does produce an excess of mental disturbance, that excess is chiefly to be found in old age (and in youth, a point to be discussed shortly). When the faculties for meeting the problems which social change can bring are reduced, then naturally it seems probable that some disturbance will be produced. However, while the point must be granted, it must be noted that the expected increase in disturbance does not always appear. Bellin and Hardt have shown that loss of the marriage partner is not necessarily a factor in the mental health of old people in the United States (4) although this must surely be perceived as social change by the subject. Again, in Canada it has been shown (although I find

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it difficult to follow the mathematics) that while certain forms of geographic change do seem to lower mental health, the act of retiring from work, which means a marked change on one's face-to-face society, does not affect the particular index that was used at all (11). Finally, while immigrants over 55 in Singapore probably did have an excess of mental hospitalization as compared with the native-born, it seemed even more probable that those who belonged to extended families or to a particular type of club did not show this excess (61). These exceptions suggest that the factor of senile deterioration is not in itself so important in relation to social change; there must be some intervening variable and this variable does not seem to be personal perceptions.

A third personal factor consists in the way in which the change is met. Both Weinberg (86) and Mezaros (58) have noted that the greatest degree of failure and of mental disturbance in their samples was to be found in those of a passive personality type, even though in the Hungarian group (Mezaros'), more active personalities might show more apparent disturbance in the early phase. From Ravn's report on the breakdown occasioned by the German surrender in Denmark the same conclusion can be drawn: it was the passive type of individual who had become involved in political matters, rather than the active collaborator or resistance worker, who predominated among his patients (70).

No other personal factors present themselves obviously in our material. Inherited traits and prior disturbances are probably relevant, but not necessarily so. It is easy to record that some suspicious circumstances were found in the life and family histories of patients who broke down while exposed to social change, but such writers never show how frequently the same type of circumstance appears in those who break down without exposure to change, or who are exposed to the relevant change but do not break down. On the other hand, in at least one type of social change it has been stated that it was disproportionately those with no family or personal history of predis-

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position who broke down (67). Sex does not appear to be a factor. Intelligence probably is but has never been demonstrated to be so. The reason for the extra vulnerability of the young adult, noted above, may be a personal matter, but my own inclination is to see this as a matter of social roles, social expectations, etc., and hence belonging more to the next section.

Social Factors. One important social factor which I think can be deduced from the studies on old age mentioned earlier as well as from other sources, is that of social expectations. When an old person experiences social change and his faculties are not such as to permit him to change himself, his behavior may become inappropriate to the new situation. This, however, need not result in mental disease unless there is a social demand or social expectation that he must change himself and that his behavior must be socially appropriate.[10] If society recognizes the old person's limited ability to change or accords the old the privilege of not conforming (as happens in some degree in cultures such as the Chinese) then it seems probable that social change as a general experience will not be traumatic. Similarly, if the young man were permitted to adjust to change at his own rate and not expected to respond in an adult fashion before he has the knowledge of life that an older man has, then we might find this age group would show no greater vulnerability to change than any other.

Social expectations are important here not only—and not mainly—with respect to age roles, however; they probably are a significant factor in quite a number of other situations we have been considering. The expectation of acculturation with which immigrants to the U.S. were formerly met, and the absence of such expectation in Singapore, is probably relevant to the native-immigrant rate differentials in each country and possibly also to the shift which appears to have occurred over the years in the United States itself. Similarly, the changing

[10] Support for this assumption is reported to be accumulating in the Duke University Studies on Mental Adjustment in Old Age (Professor E. W. Busse).

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expectations which the Negro section of the United States population has had for its members are probably relevant for the finding that Negro rates of mental hospitalization in Virginia have been rising more rapidly than white rates even though Negro social rights and status have improved (89).

Expectations are not all, however. There are other factors which seem frequently to play a part in making change a hazard, and still others which seem able to make change beneficial (an effect which is often ignored).

Closely allied to the factor of social expectations is that of social assessment. The Okinawan in his homeland is an individual among other Okinawans, assessed by his neighbors according to personal and family traits and history. When he leaves his homeland, however, he is likely to come in contact with a Japanese-originated belief that Okinawans are *Untermensch,* an opinion which is distasteful to him, which he cannot wholly reject, which he has not been brought up to accept, and which poses a problem of self-restitution that has no easy solution. To this extent he stands at the opposite extreme to the Japanese and Japanese-American migrant to the Chicago area during the last war. The latter possessed cultural traits which were falsely identified by the surrounding American middle-class as ones to which prestige was customarily given. He thus not only found it easy to be accepted (where there was no prior prejudice against him) but from the environment's perception of his traits tended to acquire opinions regarding his own status which were gratifying and which counterbalanced such frustrations as the new situation brought.[11] Hence he is reported to have adjusted better than some European groups and, at least judging from the TAT reports, was not mentally disturbed (15). Probably this mechanism of identification with society's assessment of one's group accounts for much of the marked difference in mental hospitalization rates which the East Indian demonstrates in different countries,

[11] This is Caudill's own conclusion; the impression that the mental health of the group was also good is the writer's.

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for in Africa and British Guiana (23) they had a mass of people to whom, following the predominant white attitude, they could regard themselves as superior—the Africans and Amerindians—whereas in Singapore, Fiji, and some other locations where high Indian rates have been suggested, no such reserved status exists for them.[12]

The Japanese-Americans were aided in meeting change by what Caudill regards as social misconceptions about themselves, but it seems reasonable to expect that deliberate efforts by society to assist the individual to adjust to some change would have a similar or even better effect. One interpretation of schizophrenia is that it constitutes a pathological solution to a life problem, and on this theory it would seem probable that where tradition or current society taught, and did not obstruct, a successful and healthy solution to whatever problem a particular social change has brought, then no rise in schizophrenia would occur. Similarly, it has been suggested that immigrants are more liable to the CASSP group of disorders because they must make a greater effort during most of their life to attain customary ends—effort at problem-solving one might call it. If this were so, then again it should make a great difference to the risk of getting such disorders if tradition or current authority teaches clear ways of meeting a particular problem. Conversely, however, if society avoids defining a problem which change has brought (and not only new problems, of course) and avoids discussing the possible solutions, then the risk of pathological solutions or of excessive effort seems much greater.

An illustration of this difference relates to the British P.O.W. When their resettlement problems tended to be ignored by the home communities the result was that relatively many got disturbed, but when their problems were tackled in "Civil Resettlement Units" there resulted considerable improvement

[12] It might be asked why whites in the tropics tend to have raised rates of breakdown, since they have even more people to look down on. A partial answer is that where social standing can be taken for granted, with no sense of the 'white man's burden,' then rates are low, but today the White is usually too ambivalent about his right to higher status to gain any benefit from it.

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in the subsequent mental health of those P.O.W.'s who had passed through these units (61), an improvement which might in part have taken place even if the interest and aid had assumed quite a different form. Another illustration comes from the Indian Army, where a low level of mental breakdown was noted in soldiers coming from subcultures where the power of the patriarch or local leader was traditionally strong. Here the previous pattern of submission to the chief could easily be converted into submission to, and trust in, their officers. In contrast, much higher rates of breakdown were found in Indians coming from the South, where local leaders have less tradition behind them and the patriarch, if there is one, tends to rule by force or through joint consent rather than by simple acceptance of his authority (88). A third illustration can be seen in the finding that Malays in Singapore had high rates of mental hospitalization when they followed a commercial occupation, whereas this occupation was associated with notably low rates in the Chinese and Indians. The difference stemmed, in part at least, from the fact that Malays had no tradition of commercial competition, while this did exist for the others (61).

It will be appreciated that the social factors which are being mentioned now are not ones which appear exclusively or predominantly in association with change. The image which society has of an individual, the expectations which it holds out for him, the aid which it offers in the solving of problems—these are all factors which affect mental health whether in company with social change or without it. The final factor which will be mentioned here is similar: it is the values which society puts on different experiences and actions. It must be admitted at this point that all the social factors which have been mentioned are ones which are not easily amenable to identification, comparison, or measurement by traditional epidemiological techniques, and to move from the assessment of age as a factor to the assessment of values as a factor calls for a great technological jump. The next study to be mentioned, however, illustrates one direction in which the jump might be attempted.

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In the writer's opinion it is through similar attempts that this side of the epidemiology of mental disorder will have to advance.

The work just referred to is the Cornell medical life histories study led by Hinkle and Wolff (39). One of the main conclusions which they offer has considerable relevance here. They say: "The great majority of the clusters of illness episodes that occurred in the lives of every group occurred at times when they perceived their life situations to be unsatisfying, threatening, overdemanding and productive of conflict, and they could make no satisfactory adaptation to these situations." An important inference from this is that where a change in life situation occurred and a satisfactory mode of adaptation was discovered, no particular excess of illness developed. (It must be an inference at present, for in none of the published papers on the project's findings is this specifically demonstrated to be true.) Another inference, this time drawn from individual case history diagrams which they have published, is that where such a cluster of illnesses is occurring social change may result in its cessation and hence in improved health. However, of still more relevance is their finding that "The relevant variable . . . is not the 'actual' environment and the 'actual' experiences themselves, but the subjects' perceptions of these." In other words, it is not social change, but how such social change is perceived, which is relevant for health, and while such perception is to some extent idiosyncratic, to a large extent it is determined by what society teaches. When Herodotus has Darius invite the Greeks to eat their dead fathers instead of burning them and the Gallatians to burn their dead fathers instead of eating them, and has him receiving horrified protests from each, he points a moral which is still surprisingly often forgotten today, namely the social or cultural relativity of values. Many of the life situations which are perceived as "unsatisfactory," "threatening," etc., by individuals in one society would not be perceived thus by people in another. To take the simplest examples: The introduction of modern contraceptive methods to

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non-Western peoples may in one case result in considerable relief in mental distress and in another case result in considerable augmentation of such distress, according to how the prevention of pregnancy is viewed by the different traditions. The discovery that his wife can make more money in New York than he can would probably have a very different impact on the mental health of a Southern Negro migrant than upon that of a Puerto Rican migrant. In Singapore, to have many possessions was of importance to the Chinese since it helped the perpetuation of the family, but not to the Malay, whose values were much more focused on emotional gratifications than upon security and material gratifications. The Chinese showed a marked inverse correlation between mental hospitalization and social class; the Malays showed none (61).

Types of Breakdown Particularly Associated with Social Change

Although social change does not have a pure effect on mental health in general, it does seem to be associated with certain types of breakdown. Of these the most notable, because it is a condition which is otherwise rather rare, is the acute confusional state. This has been described as especially increased in students in their first year at college (13), in immigrants just arriving off their ship (63, 60), in recently inducted soldiers (48), in foreigners lacking a knowledge of the local language (46), and as being increased in a number of countries (including Norway) (65) during a war. Depending on circumstances it may take on a paranoid or depressive coloring, but its especial characteristic is the good prognosis. Carlson and her colleagues have suggested that it may develop as a defense against rage, this rage developing in response to the insecurity which change brings to prior conflict situations (13). It appears most commonly in the young, in the uneducated, in those who are alone; and as a generalization one might say that it appears especially in those who have not been taught, or have not learned, techniques for meeting new situations. Associated factors are physi-

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cal illness or debility and lack of adequate means of communication. When properly handled it disappears quickly, and prevention seems quite possible through the provision of better guidance for the meeting of the new situation.

Another even rarer condition, but probably more closely associated with social change than the confusional state, is epidemic hysteria.[13] The majority of instances of this condition included by Gruenberg in his recent article on the subject (34), and all other instances known to the writer, could be said to have appeared subsequent to some social change affecting the community. For instance, the first great burst of epidemic states in convents that Calmeil has recorded (12) occurred throughout Christendom between 1550 and 1600, just at the time when Europe was being shaken by the Reformation, Counter-reformation, and by the changes within the church which brought these movements forth. The Vailala madness was a clear response to the demonstration of Western power (34). The outbreak of pseudo-epilepsy in Yugoslav partisans occurred when war pressures were released and readjustment to the problems of peacetime was called for (66). Often such outbreaks can be interpreted as socio-pathological solutions to some problem which tradition prevents being solved in a more adaptive fashion and the others, as in the Yugoslav case, can be interpreted as mimetic individual efforts at an individual solution.

Among the more familiar conditions, mild depression, schizophrenia, and arteriosclerotic psychosis have all frequently been reported in association with social change, but it should be noted that change can also cause a reduction in their frequency. Depression, for instance, has been described in ex-P.O.W.'s, in internees, in immigrants, and in those away from their country on study tour (52), but if one were to take types of change which lead to an addition to an individual's 'true' society rather than to a temporary reduction of it, then the reverse influence

[13] Hysteria is not necessarily the best word here, but it is the most commonly used term.

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should logically appear. Schizophrenia tends to be increased in association with social change where there is an additional expectation to adapt to the change and no clear guide as to how to do so, but in wartime Norway manifest schizophrenia was reduced, possibly because expectations regarding social behavior were also reduced; and in Singapore immigrants had less schizophrenia than the native-born probably in part because no new expectations were imposed on the former. Whether arteriosclerotic psychosis can be similarly reduced by certain types of social change is less easy to say, for at the present time the general direction of social changes is to impose new social burdens on man rather than to relieve him of them, and one theory, at least, proposes that additional mental burdens means additional risk of this condition. Since the effect is presumably a delayed one, research into the question is not easy, but some of the Singapore findings with regard to membership of extended families and of social clubs suggests that this condition is considerably preventable where the change (in this case immigration) is accompanied by an increased sharing of responsibility among adults.

The paranoid tendencies which have been described in certain types of European immigrant are not sufficiently affected by other forms of social change to be specially featured here.

General Considerations

There are a number of questions pointed up by this survey which call for investigation in the future. The first noted was the degree to which other variables like occupational status or self-selection were affecting the data on migration. A second was whether the apparently heightened rates of mental disorder in Westernizing sections of non-Western peoples was related mainly to the process of cultural change, or to the nature of the Western culture towards which the change is directed, or to other aspects of the situation as the deliberate frustration by those in power of efforts to become Westernized. A third question was the definition and investigation of the vari-

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ous social factors such as expectations, value systems and indoctrinated modes of social perception. Techniques for providing an answer to the first question are known. Answer to the second must probably depend on the collection of sufficient mental health data from the numerous locations where such cultural change is taking place; but this again calls for no particularly new techniques. The third question, however, requires types of information for which new methods of collection and new standards of assessment are required—new, at any rate, in epidemiology. While the Stirling County study and the Cornell life histories project offer some guidance as to means of collection, means of assessment are still largely beyond us and it is questionable whether the social sciences are better placed to guide us. It is to them, however, that the problem should initially be handed.

The corresponding problem in medicine that arises from this survey is the assessment of superior health. In discussing the effect of social change on mental health it has been necessary to concentrate, virtually exclusively, on the presence or absence of disease. However, social change, and almost all the other types of factors with which this conference deals, can have two types of effect: it can shift the whole spectrum of health to the right or to the left, and it can also—at least theoretically—broaden or narrow that spectrum. If we only consider the possibility of a shift to right or left, then shifts noted at any part of the spectrum should be valid for the whole of it. However, should the other type of effect be admitted, then measurements in any single part of the spectrum are no longer unambiguous: a shift to the left in a part of the left-hand side of the spectrum may mean a total shift to the left (e.g. a general worsening of mental health) but it might also mean a broadening of the spectrum with the center staying where it was (i.e. an increase in both the most unhealthy and the superiorly healthy). It must always be medicine's task to reduce the amount of disease, but it makes a difference to social policy to know whether a certain type of projected hazardous change is likely to decrease

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mental health generally or to increase the number of both the sick and the superiorly healthy. If the latter result is forecast, then the risk may be taken with a better conscience.

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DISCUSSION

DR. E. GARTLY JACO: I wish to congratulate Dr. Murphy, on behalf of the members of the conference, for preparing a most stimulating paper on a very complex subject.

I would like to outline what I think Dr. Murphy is presenting and then I would like to add, if I may, some other suggestions related to this topic. As I see it, there are two major ideas or theories expressed in this excellent paper. The first is the idea that if social change itself is pathogenic, then mental illness will increase for those experiencing greater change. Secondly, if social change is pathogenic

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only when combined with other factors or conditions, then these other conditions, and not change itself, should be investigated.

These are what I would call *etiologic* hypotheses in contrast to *epidemiologic* hypotheses. I will come back to this point later. There are four major factors that Dr. Murphy has assumed to be indicative of social change. One is immigration or international migration; the second is internal migration; the third is the impact of war and peace; and the fourth is acculturation to Western civilization. In almost all cases, hospitalization rates were the major quantitative data used to test these four factors with regard to mental illness. This is probably one reason why it is no surprise that equivocal and inconclusive results were obtained for all these four factors as related to social change. Hospitalization rates are unreliable indices of incidence and prevalence of disease, especially mental. As such, they are subject to distortion and bias from a variety of factors, and make poor sources of data to test epidemiological, not to mention etiological, hypotheses.

Turning to change itself, if we can accept a broad definition of social change, then we would also include under this rubric cultural and personal change. That is, social change may range from a change in a dyadic, one-to-one social relationship (such as between patient and therapist, or husband and wife, or parent and child) up to the establishment of a new religion, a new government or economic system in the total society. This means that social change is a very complex and extensive subject, should one conceptualize this kind of approach in examining its components and consequences.

I wish some other facets of change had been included in this paper. One I think to be important is industrialization, which is implied to some extent in the factor of acculturation. The Industrial Revolution has been a most significant force in changing not only the Western world but also other parts of the world, and will apparently continue to be so in the future.

Another factor would be urbanization—the expansion of the urban community everywhere, usually following in the wake of industrialization.

Also, I would regard social mobility as a very important instrument of social change, especially inter-generational mobility which has recently been receiving increased attention. Changes in the family life cycle from one type to another, such as moving from the

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family of orientation into the family of procreation, and eventually into the family of gerontation (the family of old age) might develop clues of significance.

The types of mobility we call horizontal and vertical social mobility could well be an important topic. These have been discussed to some extent in some of the other papers. I am not talking here about the drift hypothesis with its dimension of spatial mobility, but about whether or not one achieves or loses social status.

For example, in my own survey of psychoses in Texas, there is one finding I would like to present to the group for discussion. This is the effect of education on the incidence rates of psychosis for certain subcultural groups in that population. I found that the incidence rate of psychosis increased consistently with level of education *only* for the Latin-American (Mexican) group, and for the non-white (Negro) group, but was not found for the dominant Anglo-American group, where no correlation between education and incidence was obtained.

If we can, as we do in sociology, accept education as a channel of vertical mobility in our society, then a member of a minority group who uses education to advance himself up the social ladder, might be under tremendous stress and it would certainly be indicative of a change in his social status. The percentage of college educated Latin-Americans and Negroes in Texas is very small and these individuals would certainly be deviants in this respect. I might add, too, that the *kinds* of mental disorders found were correlated with education in both of these subcultural groups. The most outstanding instance was the very positive correlation between the rate of high toxic psychoses and education, which stood out above all the other types of psychoses; therefore, I think social mobility is a very important aspect of social change in its impact upon the individual—the index case.

Other instruments of change could be added, such as the impact of cataclysms, catastrophies, disasters, and revolution and reform.

I would like to raise a methodological point, although realizing that it may not be a widely accepted one. I would question the attempt in this paper to use epidemiologic data, such as hospitalization rates, to test etiologic hypotheses.

I think this is a misuse of epidemiologic data because I think one should use epidemiologic data to test epidemiologic hypotheses, and

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etiologic data to test etiologic hypotheses. One must re-formulate epidemiologic hypotheses for etiology and derive a different type of data to test etiologic hypotheses. The basic datum of epidemiology is the *rate;* of etiology the *case.* The former rely upon probability statements about groups or populations; the latter on "causal" statements about the individual.

I would also question the attempt to measure or to test social change in terms of personal change. For example, what happens to an immigrant as a migrating individual may or may not be indicative of social change due to migration, or to a migrating society, or to a group which loses or gains population by out-migrants or in-migrants.

I would like to discuss some other concepts of social change as they may bear on mental illness rates.

Not every component of society, as Dr. Murphy pointed out, is affected by social change occurring in that society; there is always a differential exposure to these stresses of change. This is important in evaluating the effects of change in psychopathology. It is quite possible that certain types of change may be pathogenic while other types might be therapeutic. There is one theory which holds that social change occurs because the existing society no longer is meeting the "needs" of its members. Failure to meet the needs of the group implies a pathological condition conducive to bringing about change or establishing a healthy condition where these needs are met by the new social system supplanting the old. If this theory is valid, then some types of change might be therapeutic as well as pathogenic, depending upon their consequences.

One could look at social change in another way. It might be that the factors that are conducive to change are also the ones that contribute to mental illness. Instead of change itself bringing about mental illness, those very conditions that brought about change might be the ones that affect the risks of acquiring a mental illness. I think we should look into the causes of change to see if they might also be causes of mental illness rather than look at the change itself, after its conditions have been met and its consequences faced.

What about the individual's role in social change? Are persons who are predisposed to mental illness more likely to contribute to social change than others? There have been some studies of the personalities of so-called leaders of social movements, political leaders, leaders in wartime, and others that tend to suggest this. These

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people may have a predisposing type of personality that may either make them more susceptible to promote change or, because of their incipient mental disturbance or disorganization, might do something about changing social conditions for others.

One could discuss a great variety of factors in this very broad topic. One could also offer some hypotheses open to research or discussion. If we can accept the possibility that not all social change causes personal change, then not all forms of social change are likely to have an effect on psychopathology. This I put in hypothetical form.

One could raise the question: Are certain functional illnesses in themselves defenses or resistances to social change? It is quite possible that certain functional illnesses are *symptoms* of resistance to change, either change in a social relationship within the family, or in the occupational sphere, or in any other significant, meaningful, personal sphere of human behavior.

The frequency of functional illnesses, therefore, might be an index of change going on, or the amount of resistance to or defense against change in certain social spheres. Anxiety behavior certainly could reflect anticipation of an impending change, perhaps a premature effort to cope with change or to be prepared for its consequences. In this case, we are using social change in terms of an interpersonal relationship, as well as in broader areas of human behavior.

SUMMARY OF DISCUSSION

1. Dr. Murphy deliberately did not define social change in his paper. The absence of such a definition, however, created basic difficulties in understanding which particular kinds of influences were being referred to. It was felt that a clear-cut definition was needed on what was meant, and what was not meant, by the term "social change." Thus, whatever the definition, everyone undergoes social change at one time or another. Yet there exist periods in the lives of individuals when the *absence* of social change might be more disturbing than its occurrence, such as when the man who, desiring a promotion which would bring with it increased social status and higher living standards, fails to get it.

In answer to this point, it was suggested that some terms did not

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need a precise definition of what they referred to since they developed, instead, a proper appreciation of the framework within which they might usefully be considered. Such terms as "socio-economic class," "poor hygiene" and "social change" are convenient ways of grouping together very roughly related phenomena. They are not unitary factors but constellations of factors, so that when attempts are made to find causal relationships these terms have to be broken down into their component elements. They are useful for marking rather broad targets in order to see which particular components seem to be related to which particular kinds of disorders and in what combinations.

The concept, as a general label, helps guide and interpret particular observations, but the concept cannot be directly observed. Thus, while it can be noted that some persons do not brush their teeth or take baths, one is not observing "poor hygiene" as such. The concept forms part of the interpretation of particular observations; and these in turn form an operational definition of the use of the concept. The error is to forget the observations and to report directly in terms of interpretation—an error of pre-classification.

2. The term "migration," like "social change" is a category embracing many different types of phenomena. For any specific migration, it was important to examine the actual context in which it occurred, since at various times and places two distinct mechanisms were at work: involuntary migration (ranging from slavery and taxpeonage to political refugees) and voluntary (including mass labor recruitment and individual self-selection).

If much has been written about mental disorder in migrant populations under the heading of social change, far less has been reported about the nature of the populations that remained at home. There were suggestions that in these non-migrating populations there existed a reservoir of considerable pathology, including psychopathology, and one hypothesis proposed that it was the amount of pathology that was present that determined whether a person was able to make the decision to migrate.

3. Various methodological points were raised on the investigation of social change and migration in relation to mental disorders. The study of immigrant groups presented characteristic difficulties because they could not be considered representative samples of the populations from which they came. Usually it was not practicable

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to observe how they differed psychologically or physically from their parent populations, and most studies only compared immigrants with the population of an area receiving them. Dr. Jaco's study of Mexican immigrants to Texas was such an example. He described two unexpected findings which turned up when his data on incidence rates for psychosis were adjusted for age and sex to make them comparable to U.S. born persons of Mexican parentage in Texas. While the crude rates for immigrants were higher than for the native born, the age-adjusted male rates for immigrants were lower and the rates for females were higher, than their respective U.S. born controls. Dr. Jaco offered an explanation in cultural terms: The male migrant considered immigration an achievement, while for the female, immigration meant loss of identity bound up within the traditional Mexican family structure.

As a suggestion for a possible study comparing psychiatric illness in the successful migrant with the unsuccessful, the emigration of West Indians to both London and New York raises the standard major problems: How and where is the process of self-selection to be studied? How are rates to be developed when population denominators do not exist?

4. It was noted that these inherent methodological difficulties which were involved in working with immigrants could be avoided by studying internal migration—that is, migration within a single country. As an example, coal mining areas in Britain which had experienced shifts in population over a period of time showed an interesting variation in death rates which related to those areas having the heaviest emigration. These areas, while they showed the lowest suicide rates, had the highest death rates from heart and lung disease.

These findings, while they might suggest a number of explanations, required field surveys to test out hypotheses. Cochrane, again studying coal miners, examined both migrant and remaining populations. He found that the physical condition—as measured in terms of lung function—of the miners who remained in the South Wales coal areas, was much worse than that of the men who had emigrated to new coal mining areas in other parts of the country.

5. While such clinical case studies have their limitations, they allowed solutions to the problems of measurement that are better than the usual comparisons of mortality or morbidity rates that are to be

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found in broad population studies. Methodologically, it is necessary to devise a case-finding technique that is not affected by the social changes under study—namely, migration. For it would be most extraordinary that an immigrant population would have the same relationship to clinical services in the new country as it had had at home. This seems to underline the point that data on migrants derived from routine clinical contact were rarely, if ever, adequate. What appears necessary is to set up case-finding survey teams dissociated both from the migrant and from the non-migrant populations under study.

Yet to leave such social institutions as hospitals with their established reporting systems was to become mired in a swamp of diagnostic problems. In his paper, Dr. Murphy acknowledged that the methodology that had produced existing data had generally been so crude that it was not really possible to establish the facts. So that, were he to have included an expanded definition of mental disorder which encompassed such forms as delinquency, or general anxiety, or the entire series of presumably psycho-physiologic disorders listed in the APA nomenclature, he would not have been able to use even the crude measuring stick of hospital admissions. For there existed no standards of comparison; not even a methodology for arriving at a standard. It was a problem that had long engaged such conferences as these: What was mental disease? What was a case?

6. Replying to the last two questions, Dr. Leighton reviewed some studies that he and his colleagues at Cornell had been working on. From these has come the concept that every community—viewed as a socio-cultural system or quasi-organism—has a "threshold of tolerance of change" which is related to the *amount* of change (whether benign or not) that occurs within a certain length of time. When the rate of change extends beyond the threshold of tolerance, the community begins to fall apart, failing in such functions as the socialization of children or the maintenance of economic enterprises. While some disjunction occurs with any change, the threshold is that point at which the changes become so great that they develop into a vicious circle—a spiral of more and more disintegration—independent of the factors that induced the changes in the first place. It is believed that the population of such communities would, in trying to cope with their situation, develop more mental illness than the population of communities below the threshold. The fundamental prob-

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lem, as has been noted, is to find standards of comparison that would hold say, for villages in such disparate places as Burma, Japan and Peru.

Two possible frames of reference were suggested for this problem of cross-cultural comparisons of phychiatric disorder. One would be to use the psychiatric definitions now current in the European-American cultural system to see what patterns of disorder existed. Such an approach does permit appraisals without too much difficulty of such conditions as mental retardation or even schizophrenia. However, to attempt to assess personality disorders, psychoneuroses, and sociopathic behavior, would raise some very real problems. The other frame of reference would be to adopt the definitions of deviant behavior that are native to the culture under study—to work with its own concepts of what is queer, strange, abnormal, etc. In practice, Dr. Leighton concluded, the best course was to develop criteria using both frames of reference.

7. The opinion that epidemiological data was not suitable for testing etiologic hypotheses, was disputed. In support of this view it was maintained that, while actual causes operating at the level of the individual case were reflected in group data in the form of rates or probabilities, these data could only suggest hypotheses of cause and effect but do not constitute a test of such hypotheses. In opposition, it was maintained that epidemiology, as the study of occurrence of disease, is necessarily concerned with disease etiology. Classic epidemiology studies, such as Baker on Devonshire Colic and Goldberger on pellagra, were cited as instances in which etiologic knowledge was gained through the epidemiologic approach.

8. Dr. Murphy was asked to discuss the statement he made in his paper that ". . . in wartime Norway schizophrenia was reduced, possibly because expectations regarding social behavior were also reduced. . ." What was the evidence for this reduction, and what was the evidence for the imputed cause?

DR. MURPHY: There are three main strands, I think, in the discussion. One is the question whether we can usefully have any discussion at all when we do not have definitions of the topics being discussed. The second is methodology. And the third is the question whether social changes are useful things to generalize about at all.

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On this last point, the aim of my paper was to see whether generalizations about social change seemed useful or not. We always seek the widest generalizations possible in research in the hope that findings from one area may thereby be made useful in others. There are plenty of people in the past who have thought of social change as encompassing all the various things I have referred to, but this does not mean that the concept is useful to us. Perhaps bringing them all under the one rubric is wrong. I have not, myself, come to any answer on the point. However, the general approach is the one which we customarily use, and I think it is the one which we should continue to try and achieve, by seeing what degree of generalization is possible and what goes too far.

Now, as to definition: there have been I don't know how many different attempts in the past (though not in medicine) to define social change. I cannot attempt to mention them all, but one which may have a little relevance to the way our discussion was going is that of Arnold Toynbee. He divides, in a very definite manner, countries and peoples into those which have, and those which have not, a 'history.' By this he means that there are countries or peoples whose lives go round in circles. They may have petty wars, etc., but essentially they return afterwards to the same point that they started from and hence their story is all repetition with change hardly entering in. In contrast to these, he singles out the major cultures or major civilizations as having history and hence as experiencing change. When they go through a war they do not return to where they started, but rather take a further step on what he claims to be the long cycle of development and decay. We might similarly view social happenings as change, or as not change, according to whether they seem to lead on in some process or merely permit the subjects eventually to return to their point of departure.

Another way in which we might look at it is by distinguishing between changes which the past history or past teaching of a people have given some idea of how to solve, and changes for which the past offers little or no guidance regarding solution. I would think that it is this group which most suitably falls into the category of real or serious social change from our point of view.

The question of defining social change according to the purpose that one has in view requires a whole paper. In my paper I have mixed up many levels, which has been justifiably criticized (Point

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1). At times the paper operates on the personal level but most of the time on the community or population level, and these admittedly need to be distinguished in a way that I just did not trouble to do. Just how we can move over from one level into the other, and just how we can relate one category to the other is an interesting problem which needs study. We should, in other words, be looking into the question of how far clinical data can be used for answering problems which arise from epidemiological material and vice versa. However, I don't agree at all with the question of the division between epidemiological data and etiological data (Point 7.). I think data are data. The difference is not one of types of data but of types of hypotheses or types of logic. It is really the classic difference between inductive logic and deductive logic.

Epidemiological data are observed facts, and as such can be used to test hypotheses in the ordinary fashion. One forms an hypothesis and then asks oneself—Under what sort of situation would I expect, from this hypothesis, certain differences or certain patterns to appear. So one hunts through the available types of data to see if the type of situation which one requires for the testing can be found and if data on it are available. That is the way in which I believe epidemiological data can be best used for testing, and in terms of deductive logic I think such tests do offer proof insofar as proof is ever possible at all. (For, of course, there is a school of science which believes that absolute proof is never possible. All that is possible in this theory is the attaining of a further step in an evolving knowledge, a step which will be corrected in turn, at the next level that the body of knowledge reaches.)

On the question of method, which a number of people have referred to (Points 3, 4, 5, 6), I may say that this has given me considerable concern recently, and that I am at the moment designing a study to try and meet many of the points which have been raised. It might be of a little interest if I sketched it briefly, since it indicates how I think the problem should be tackled in one particular sector, immigration.

What I want to try and do is compare a random sample of prospective, accepted emigrants from a country with a matched sample of people who have not declared any intention to emigrate; and then with the same instruments compare these with population samples in a country of resettlement, or a series of countries or resettlement.

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The latter samples would in part be natives, in part immigrants from the homeland originally studied, and in part from some other homeland. The core of the study would be material collected through directed interviews of the various samples, but this would be augmented by standard epidemiological data on mental hospitalization, suicide, delinquency and crime for the various populations being sampled, and there would also be a psychiatric interviewing of smaller samples. This is the way in which I see the problem which Dr. Carstairs and others have posed, and the way in which I see it as being answered; however, I have no money for the study yet.

I think a specific question was asked on the source for my statement about schizophrenia being reduced in wartime Norway (Point 8.) This is Ødegaard's paper on wartime incidence[14] and it refers to hospitalization, of course, not to so-called true incidence. His analysis of the estimated duration of the disease prior to admission in this same group suggests that there was no increased delay and hence that there was a true decline in schizophrenia in the community. It is debatable why it occurred and there is no evidence on the subject, so that it is purely my own hypothesis that I offered in my paper.

I don't think there were any other questions specifically addressed to me, and so I think that is all I have to say.

[14] Reference (65).

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